JOURNAL

OF THE

ELISHA MITCHELL SCIENTIFIC SOCIETY

1894

ELEVENTH YEAR PART SECOND

JOURNAL

OF THE

Elisha Mitchell Scientific Society

VOLUME XI—PART SECOND

JULY--DECEMBER

1894

POST OFFICE:
CHAPEL HILL, N. C.

ISSUED FROM THE UNIVERSITY PRESSES.
CHAPEL HILL, N. C.
1894.

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HISTORY OF THE ATLANTIC SHORE LINE.*

HUNTER L. HARRIS.

The history of a shore line consists in an exposition of the changes which have taken place in it; these changes consisting chiefly in its migration across the land surface on which the body of water rests. If this body of water be an ocean or in direct and open communication with an ocean these changes of position may be effected in two possible ways: (1) By an actual depression or elevation of the water surface. (2) By depession or elevation of the land mass along which the shore line occurs.

Without discussing the reasons for such a conclusion we may say that in the great majority of cases the first of these two causes need not be considered as a factor. Usually it is the oscillation of the crust about

* This paper is a thesis prepared in the second course in Geology at Harvard College, and has been furnishrd me by Mr. J. B. Woodworth, the instructor under whose direction the work was done. It is an admirable compilation, and serves as an introduction to the more extended work upon which Mr. Harris had entered at the time of his death.—C. C.

the margin of the ocean that causes the migration. If this be in the nature of an uplift the sea will recede, the shore line successively occupying positions further and further out upon what was formerly sea-bottom. If on the other hand a subsidence of the land surface takes place, the sea will transgress the land and the shore line will successively occupy higher and higher parts of the land slope, that is, further and further inward from its former position.

Slight changes in the position of a coast line may take place in other ways than by bodily movements of the land mass, namely by the deposition of material on the margin of the sea-bottom, thus causing the shore line to recede from the land and by removal of material from the shore, thus causing the sea to transgress the land. These causes are however productive only of comparatively small migrations when acting alone. Evidently we must look upon oscillations of the land mass as the chief cause of change in the position of shore lines.

In order to get at the history of such a shore line as that of the Atlantic of North America, we must know how to read the evidence of its former presence in places other than that now occupied by it. What are these evidences? Probably the most direct, as well as the most exact, evidence is furnished by the actual and characteristic marks left in the form of raised beaches or bench marks. Another evidence is furnished by the position and character of sedimentary deposits,—though the absence of evidence of either kind does not necessarily mean that shore conditions were never present there. While the presence of beach marks furnishes more exact evidence of shore line position in certain cases, the application is not of so wide extent as

the more general evidence of sedimentary deposits, on account of the greater ease with which the former are effaced. The burden of the evidence then lies in the sedimentary deposits of the Atlantic slope, or, more accurately, of that part of North America whose sedimentary deposits can not be referred to some other ocean or water body.

To decide what should be taken as evidence of proximity to shore line we must be familiar with the principles governing the deposition of sediment. Running water is the principal carrier of fragmental materials such as go to make up secondary deposits. These materials vary greatly in coarseness and in composition. Far the greater part, however, is mineral matter, resulting from the decay and disintegration of the rocks of the land, and the fragments which compose it vary from an impalpable powder to the greatest size which can be swept along by water in motion—this latter depending upon the velocity and volume of the current. It is by means of this principle of the carrying power of water that we explain the sorting of those fragments which find their way into moving water. A current of high velocity will transport comparatively large pieces of rock material, until, by decrease of slope or by entrance into some other body of water, its velocity is lessened; then the materials will be deposited, the coarsest first and others in turn as the stream continues to lose velocity. The finest may be deposited a great way out in the ocean, sea or lake, which receives the transporting current.

Since the ocean serves as a receptacle for all the drainage of the land, there is being deposited, within its minor depths and out to a distance of perhaps a hundred miles from the land, all the solid materials

brought into it by streams. And since the ocean provides the gradual retardation of currents which make their way into it, we have a perfect fulfillment of the conditions of water sorting, and, hence, we may declare the general rule that the coarser materials are deposited near the shore and the finer out. Indeed, where we find undoubted marine deposits including fragments of large size such as grit or pebbles, we may reckon with certainty upon the proximity of the sea shore during the time when they were deposited.

So then the presence in any region of such fragmental deposits as may be judged from their nature to be *marine* declare unmistakably the presence there of the ocean at such a date in geological history as our study of these deposits may refer them to. For instance, if we were to find in Western New Jersey marine deposits of Cretaceous age, dipping gently eastward, we should conclude that during the Cretaceous period the Atlantic shore line lay west of that point. How far west it may have stood we must determine by other means, perhaps by actual shore marks, such as a wave cut bench, or a series of beach gravels or sand dunes. Or else from from the coarseness of the sediments near their inner border we may conclude that they mark the actual shore line of that period.

Other processes of reasoning are often brought to bear which cannot be dwelt upon within the narrow limits of this paper. We should remember always that such evidences as have been mentioned do not necessarily indicate the greatest amount of encroachment of the sea within any given period, for deposits made further inland may have been removed by the general erosion of the surface: so also shore marks are comparatively seldom left as enduring monuments, and their

absence may be no evidence of the absence of shore conditions.

Of the relative position of land and water (and consequently of the shore-lines) of pre-Cambrian times we know almost nothing. Those changes, which we call metamorphism, have progressed so far, by virtue of the great age of these sediments and their position at the bottom of the stratified series, that it is extremely difficult to read in them the conditions under which they were deposited. We are quite sure that there existed, at the beginning of Cambrian times, a land area, made up of pre-Cambrian sediments, lying somewhere along the Atlantic coast region of North America. A series of very old, highly altered and disturbed sediments now exist as a land area forming an almost continuous belt between the Appalachian mountain system and the present Atlantic border from Canada southward to Georgia. This area, which is in the main supposed to be pre-Cambrian in age, is plainly shown by its structure to have been once involved in a series of complicated mountain building movements, and was in fact part of a great mountain system. Where this land once rose to mountain heights it is now a low, gently sloping and undulating surface made up of hills of gently rounded outline, all rising to about the same height and having in the distance the appearance of a flat country. That is to say it has so long been subjected to the forces of denudation that the mountain ridges which once existed have washed away and finally disappeared, leaving a land surface of low relief and weak topography. What then became of all the material thus removed? It found its way into the borders of the adjoining ocean and was laid down as sediments, and so to the east, south and west of this pre-Cambrian area

lie now upon its edges where they were deposited, the later sediments of the stratified series.

Having found out something of the position of the land of the Atlantic coast region at the initiation of the Cambrian period, we may begin to formulate our knowledge of the shore-line history from that date.

First let us observe that the evidence of former shore-lines given by actual marks of the shore itself is of such a transitory nature that we must not expect to find such evidence in the older rocks. While they may retain a perfectly characteristic form through the Quaternary period or even longer, the chances of their preservation from earlier times becomes less and less as we go back into the geological past. In the Cambrian then we are forced to reason almost entirely from the nature of the sediments, that is, their texture, and composition, and their position with respect to the source of the materials forming them.

There are very few undoubted Cambrian rocks in North America which can with any degree of certainty be ascribed to the Atlantic field of deposition. In the Cambrian Correlation Papers of C. D. Walcott, a series of rocks of Cambrian age, including slates, quartzites, conglomerates and limestones, are located and briefly described under the general name of the Atlantic coast province. These areas are rather small and discontinuous, and extend in a general southwesterly direction from the southern coast of Labrador, across Newfoundland, Nova Scotia, and ending in eastern Massachusetts. The age of each district has been determined by fossils which occur, however, only in restricted zones within the formation. The other members are often classed as Cambrian only on conjecture: hence arises considerable difficulty in interpreting the conditions of

deposition. This difficulty is increased by the removal of the greater part of these rocks by erosion, leaving widely separated patches which can scarcely be placed in any reasonable relation with each other. Usually, however, the series lie upon the eroded surfaces of pre-Cambrian rocks, Laurentian and Algonkian, and often show basal conglomorates formed from those rocks. In such cases the materials seem to have been brought from the west or northwest, and from no great distance. These areas lying about the Gulf of St. Lawrence seem to show by the gradual change in the character of the sediment from basal conglomerate to limestones formed at a moderate depth, that the sea transgressed the Algonkian land westward, allowing the accumulation thereon of the Cambrian deposits, first in shallow bays and afterward in gradually deepening water.

Regarding the Cambrian slates and quartzites of eastern Massachusetts, Prof. W. O. Crosby says; "In general the quartzite is more and the slate less abundant northwestward, indicating that the ancient shore line along which these slates were deposited lay in that direction, and originally the Primordial strata were probably spread continuously over all the region to the southwest of that line." Also, "It is very clear that the quartzite, north and west of the Boston basin, is the source of the quartzite pebbles which play such a prominent part in the composition of the conglomerate, especially in the central and northwestern sections of the basin."

Of the Cambrian section of Bristol county, Massachusetts, Prof. N. S. Shaler says: "The frequent return of conglomerate layers and the coarseness of the pebbles show that during most of the time when the beds were accumulating the region was near shore;

so too the large amount of sandy matter even in the slates affords a presumption that the region was not remote from the coast line. The rocks from which the pebbles were taken were mainly identifiable in the western portion of the field above described."

The general inference is then that during a large part of Cambrian time the shore line was in a general way coincident with the present shore-line from Massachusetts northward; that a gradual subsidence of parts of the coast region ensued by which the ocean transgressed the land, accumulating, as it moved inward, a sheet of coarse deposits which were in turn covered by fine argillacious and calcareous sediments forming slate and limestone. These seem to have been formed in a sheltered sea, hence the opinion is that the land barriers existed somewhere to the *east*. During this inward march of the shore-line there must have been many partial returns to its former position but the general result was an inward extension, amounting in some places to fifty, and in other places to one hundred miles, from its present position.

When we attempt to reckon upon the southward extension of the Cambrian shore-line we are entirely at a loss, for, in the first place, we have no known Cambrian deposits south of New England which can be clearly ascribed to the Atlantic field of deposition. Apparently the Cambrian, as well as the whole of the Paleozoic rocks are entirely missing from the southern Atlantic province. This has led to the belief, which is supported only by negative evidence, however, that during the whole of the Paleozoic era the eastern extension of the continent was much greater than it is now. There is really little doubt that this was the case, and the evidence of land barriers lying to the

east of the New England section during Cambrian times, leads to a conjecture which may here be stated.

A persistent and connected series of Cambrian outcrops lies along the Appalachian mountain system from Alabama to the river St. Lawrence. These are known to have been deposited in the great continental sea which covered the central portion of North America during the whole of Paleozoic time and even later. These Cambrian rocks with the other Paleozoic sediments were involved in the orographic movement which gave rise to the Appalachian mountains. Their present outcrop, however, is adjudged to mark in a general way the eastern border of the continental sea in which they were deposited. To furnish this enormons thickness of Paleozoic sediments a much larger land area must have existed toward the east than now remains. The fact that the denuded surface of much folded pre-Cambrian rocks is seen now to disappear eastward under the present continental shelf, in some measure bears out this idea.

The conjecture now follows, that the Cambrian rocks of New England heretofore described as belonging to the Atlantic coast province form really a part of the Appalachian province; that is, that they were deposited not in the Atlantic, but, along with the not far distant Cambrian rocks of eastern New York in the continental sea. This satisfies the conditions which have been predicated of them, namely, that they were deposited, not in the open ocean, but in a more or less sheltered sea. The elevation of a part of this area in the process of Appalachian mountain building and the subsequent denudation of the whole of New England, reducing it almost to base-level, would account for the existence of the Cambrian rocks only in isolated

patches, while the disturbance of their original stratigraphic position would make it impossible to read any of their history by the stratigraphy.

If this conjecture be true, the "land barriers lying to the east" of New England would be but a part of the broad pre-Cambrian land strip which extended from some part of the North Atlantic in a southwesterly direction almost to the present shores of the Gulf of Mexico. In such case all ideas of the Atlantic shore line previous to the Triassic period are involved in the statement that it existed *at some distance east of its present position.*

Taking up the thread of the history at the beginning of Mesozoic time we find a series of elongated basin deposits of Triassic (Rhaetic ?) date consisting chiefly of red sandstones and conglomerates. These rocks form a long train of detached areas stretching from central Massachusetts southwestward to South Carolina. They lie unconformably upon the denuded surface of the pre-Cambrian crystallines, and appear to have accumulated either in shallow inland seas or in sheltered embayments of the ocean. All of them are separated from the present ocean by older rocks, except that of Connecticut, which itself communicates with it only by a narrow neck.

We have, then, in Triassic times very little evidence of the position of the Atlantic shore-line itself. If the Triassic rocks of Connecticut and New Jersey were, as has been thought, deposited in embayed portions of the ocean waters, or fronting the open sea, then we must have had a coming in of the shore-line by submergence of the greater part of the pre-Cambrian land area, by which the Triassic sediments accumulated even upon the edges of the Paleozoic rocks of the conti-

nental province. Such a submergence must have brought the ocean to the very foot of the Appalachian mountains which had received their initial uplift just before the beginning of Triassic time. But, if on the other hand, the deposits were made in lagoon-like baisins of inland waters, the ocean shore-line may still have stood as far out as at present, or farther. The evidence is that those areas south of the New Jersey area at least, were accumulated in inland seas. If such was the case — and it is the most probable theory — the Triassic ocean extended inland in a great bay with its center somewhere near the mouth of the Hudson River and its shore-line reaching to the base of the Appalachians in western New Jersey and eastern Pennsylvania whence it swung gradually southward to somewhere near the position of the present shore-line.

The conditions of depositions of these sediments have, however, always been difficult to reconstruct. No solution has ever been offered which proved generally satisfactory. The Connecticut basin seems to represent the estuarine phase of a river which was the ancestor of the Connecticut. From analogy, I would offer as an explanation of the elongated similar basins to the southwest that they also represent drowned portions of consequent rivers which may be reasonably supposed to have existed at so short a time after the folding of the crust which formed the Appalachian system. The character of the deposits would accord well with this supposition.

Following the Triassic period of deposition came an emergence causing a retreat of that part of the shore-line south of New England, by which it assumed a position coinciding with the present shore in Long

Island Sound, but gradually departing therefrom toward the south. In Maryland the departure amounts to one hundred miles inland from the present coast and it continues at about that distance to Georgia, where it swings rapidly westward and northward forming the Mississippi embayment of the Cretacous ocean.

The extensive denudation, which had been long going on over the permanent land area, now extended over those Triassic rocks which were above sea level, and, by the beginning of Cretaceous time, this area was reduced to surface of low relief, much as it appears today, but somewhat nearer base-level. Of the conditions existing during Jurassic time we know nothing since we have no distinctly Jurassic rocks in this province. But of Cretaceous deposition we have a good record in at least two formations whose inland extension is marked by the Cretaceous shore-line already described.

A study of the earlier of these two formations by W. J. McGee shows that, after the base-levelling of the ancient land area, which was achieved just before Cretaceous times, a shoreward tilting of the area took place by which the streams were revived to such a degree that they rapidly sank into deep narrow valleys. A submergence then caused the sea to invade these estuaries, the coast assuming in general the position which marks probably the greatest transgression during Cretaceous time. Then followed the accumulation of the Potomac sediments which, with their equivalents, extend from New Jersey southward, certainly as far as North Carolina, and probably as far as the Mississippi embayment. A period of emergences and retreat of shore-line then intervened before the deposition of the glauconite beds of later Cretaceous which are seen to

overlie the Potomac. During *later* Cretaceous then the shore line returned nearly to its former position. Close study of the different members of each of these formations would probably reveal signs of shore migrations of comparatively small magnitude besides the sweeping changes herein mentioned. Moreover it is not definitely made out that the inner border of existing Cretaceous deposits is the limit of encroachments of the sea in Cretaceous time. Great denudation, going on in Tertiary time, caused a second base-level to spread over a considerable part of the ancient land area which had been uplifted at the close of the Triassic; and it is to be supposed that much of the denudation took place in the relatively soft Cretaceous beds, by which large areas may have been entirely removed.

Coming now to the Tertiary, we find that while there were undoubtedly many oscillations of level during this period, the principal Tertiary shore-line corresponds closely to that of the Cretaceous from their most northerly occurrence (off Cape Cod) as for south as Virginia. Between these points the two border lines are never more than twenty miles apart. In Southern Virginia, North Carolina, South Carolina, and part of Georgia, the Tertiary border overlays that of the Cretaceous; but from Georgia northwestward into the Mississippi embayment, the Tertiary lies further out, allowing an exposure of the Cretaceous beds in a strip perhaps fifty miles wide.

In the Tertiary of the Atlantic coast province, Eocene, Miocene, and probably Pliocene, sediments occur, though in the case of one or more formations it is difficut to discriminate between Miocene and Pliocene. Hence the term Neocene is often used to describe the Lafayette (Appomattox, Orange Sand) formation of the

middle and southern Atlantic, which has been carefully studied by Mr. McGee. There is usually more or less unconformity between the three or more formations of Tertiary, and often they are not continuous over the whole Tertiary field, but thin out and disappear from some portions while they reach great height and thickness elsewhere. It is difficult to say more than there were at least three migrations of the Tertiary coast-line caused by uplift and subsidence which took place rather unevenly but never causing any considerable transgression over the border line already described.

It is reasonable to expect that the characteristic forms caused by the persistence of shore conditions at certain levels would furnish evidence in the case of such recent deposits as the Tertiary, and doubtless they would if sufficient study had been made even of these terraces, shore cliffs and raised beaches which are known to exist. Such shore marks would enter very prominently into the investigation of the Quarternary shore-line to which our attention must now be directed.

It should be remembered that there has been a successive addition of essential land surface along the Atlantic slope from Cape Cod southward through all the geological time from Triassic down to Quaternary, and a consequeut recession of the shore-line eastward. From Cape Cod northward, however, the reverse has to some measure been true, that is, there has been an excess of subsidence over the constructive processes by which all the deposits from the beginning of the Mesozoic to the Quarternary, if formed at all, are now buried beneath the sea.

The most marked feature of Quarternary time was the great ice invasion. A prodigious accumulation of ice in the northern half of the continent was accompan-

ied by a spreading out of its mass on all sides so that it extended southward far into temperate lattitudes, reaching at its greatest extension the middle courses of the Mississippi River. On the Atlantic coast of New England the glacial covering spread into the sea and probably floated off as icebergs. As a result of the abrasion and transport of rock material by the ice, the glacial field is covered with deposits of irregular distribution and possessing the peculiar characters by which they are readily distinguished from ordinary aqueous sediments. During the formation of some of these glacial heaps, the land along the North Atlantic coast must have stood somewhat higher than at present. Some of these deposits, which were evidently made upon the land, now lie as small wasting islands off the present shores. Indeed it has been claimed that the central part of the glacial field must have stood much higher at the beginning of the glacial period than now, the difference amounting to thousands of feet in the region just south of Hudson's bay, which was supposed to have been the glacial centre. This has been supposed a necessary condition to account for the accumulations of snow and ice in such enormous quantities, and its descent into lower latitudes. There is evidence of such a condition in Europe in the fiorded Scandinavian coast, as has been pointed out by Dana, and the submerged valleys extending out from the Hudson and other rivers may be cited as an American evidence of a similar sort. But we have much clearer evidence of *submergence* during a later part of the glacial period, which amounted to only a few feet at New York City but increased northward, — reaching 200 to 225 feet off the coast of Maine, 500 to 600 feet at Montreal and 1000 feet on the coast of Labrador.

These approximate figures are taken from careful measurements by various observers of the heights of certain shore cliffs and marine deposits shown to be of Quarternary age. I have omitted measurements of various points between, and have given in the place of exact figures a sort of general average of the observations of several persons, arranged in such a way as to show the gradual increase in the amount of submergence going northward.

It is difficult to reconcile the views held by various geologists of the amount of subsidence which took place at several points along the New England coast and northward. The differences depend on different criteria used in the discrimination of shore-lines, difference in opportunity for and general incompleteness of observation, etc. The consensus of opinion, however, as regards the Atlantic shore-line is about as I have represented it. The result of such a subsidence must have been the submergence of parts of the Maine coast, parts of Nova Scotia, Newfoundland and Labrador, and of a large area extending up the St. Lawrence River to the Great Lakes.

Of the middle and southern Atlantic coast little has been done in discriminating and tracing the many shore-line terraces of Quarternary age which undoubtedly exist as distinct features. Some mapping of the Quarternary deposits has however been made. Of these, one formation has been studied by McGee and named the "Columbia" loam. It belongs especially to the middle Atlantic slope and is older than the moraine deposits of the glacial epoch. It is both fluviatile and marine and is scarcely observed southward from North Carolina, where its inner border approaches the present coast. It represents a brief

glacial submergence amounting to 400 feet in the northern part of the field (New Jersey) and almost nothing at its southern limit.

Other Quarternary formations occur in the southern field, of which the shore-line limit lies 10 to 50 miles inland, from South Carolina to Florida. Here the Quarternary forms one third of the peninsula (the southern end) and thence the division line swings along the Gulf coast where it marks off a border formation almost equal in width to the similar strip along the Atlantic states.

Far the largest area of land surface which has been added to the Atlantic border is seen to to be of Tertiary age. On the inner border of this a narrow strip of Cretaceous and on the outer edge a thin layer of Quarternary sediments, make up, taken with the Tertiary, practically all that the continent has gained; and the area represents the final amount of recession of the Atlantic shore-line during recorded geological time. North of Cape Cod the result of oscillation so far has been on the other side, and the coast-line now probably stands farther in upon the land than at the beginning of recorded time. Here we have lost rather than gained continental area.

The subjoined map attempts to represent the Atlantic shore-line history in the order in which I have attempted to compile it in this paper. The study has necessarily been crude and incomplete in its nature and is offered as an introduction to a more critical and extended study which may be undertaken later on.

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AN EXAMINATION INTO THE NATURE OF PALÆOTROCHIS

CHARLES HENRY WHITE.

While Professor Ebenezer Emmons was "Geologist to North Carolina," he discovered a singular form in Montgomery county which he believed to be a fossil. He announced the discovery in 1856, and owing to the low horizon in which he found it, regarded it as the oldest representative of the animal kingdom on the globe, and gave it the name *palæotrochis;* old messenger. At this time he wrote a letter to one of the editors of the American Journal of Science in which he said: "It is evident that the fossil is a coral," and described it as follows: "Form lenticular and circular and similar to a double cone applied base to base; surfaces grooved, grooves somewhat irregular but extended from the apices to the base or edge."[1]

In his Report of the Midland Counties of North

1. American Journal of Science, Vol. XXII, 2nd Series, p 390.

Carolina, on plate 14 opposite page xx., he gives a section of the region in which the palæotrochis is found, which is reproduced in Fig. 1. On page 61 of the same is the following descriptive section, enumerated in the ascending order:

1. Talcose slates, passing into silicious slates, and which are often obscurely brecciated. Thickness undetermined.

2. Brecciated conglomerates, 300–400 feet thick, and sometimes porphyrized.

3. Slaty Breccia, associated with hornstone.

4. Granular quartz, sometimes vitreous, and filled with fossils and silicious concretions of the size of almonds; 200–300 feet thick.

5. Slaty quartzite, with a very few fossils, about 50 feet thick.

6. Slate without fossils, 40 feet thick.

7. White quartz, more or less vetrified, filled with fossils and concretions; 700–800 feet thick.

8. Jointed granular quartz, with only a few fossils.

9. Vitrified quartz, without fossils; 30 feet thick.

10. Granular quartz, no fossils, and thickness very great, but not determined.

He says that some of the rock beds in which these forms occur consist almost entirely of them, and are intermixed with almond shaped silicious concretions "which frequently contain the fossil." He speaks of their occurrence from the size of a small pea to two inches in diameter, but by far the greater number belong to one or the other of two sizes; the smaller size represented by Figs. 2—4, and the larger by Figs. 8 and 9. The smaller he calls palæotrochis minor, and in addition to the characters given above, "the apex of the inferior size is excavated, or provided with a small

roundish cavity, with a smooth inside, or sometimes marked by light ridges, which may be accidental; the opposite side is supplied with a rounded knob, from the base of which the radiating groves begin."[1] In the larger, or palæotrochis major the rounded knob and opposite cavity are absent.

"This fossil is a silicious coralline, and not silicious from petrification. It seems never to have had a calcareous skeleton like most corallines: but, during its existence, to have been entirely composed of the former substance. The animal was gemmiferous—the germs being sometimes cast off, in which case new and independent individuals were produced; on others, the germs adhered to the parent. These start from the circular edge at the base of the cones; their growth produced a change of form which is illusrtated in Figs. 2 and 4.[2]"

"The palæotrochis is found at Troy, Montgomery county, at Zion about twelve miles south-west of Troy, where the fossil occurs in the greatest profusion. It has also been noticed on the road from Troy to Birney's bridge."[3]

Shortly after the description of the palæotrochis was published, Professor James Hall, in a letter to Professor Dana,[4] suggested that these forms were merely concretions. In 1868, Professor O. C. Marsh in an article on this subject in the American Journal of Science says that he suspected that they were inorganic and an examination of the interior clearly indica-

1. Geological Report of the Midland Counties of North Carolina E. Emmons. p.62.
2. *Ibid* page 63.
3. *Ibid* page 64.
4. American Journal of Science Vol. XLV. p. 218.

ted that they were not corals, and as soon as microscopical sections could be prepared, they were more carefully examined, but no trace of organic structure could be detected, the entire mass being evidently a finely grained quartz. It follows therefore, he says, that this name should in the future be dropped from the genera of fossils. He says further: "Admitting the inorganic nature of these remarkable forms, their origin becomes an interesting question and it is certainly not easy to give a satisfactory explanation of it," but, that it seems to have some analogy with cone-in-cone structure which is probably due to the action of pressure on concretionary structure when forming. In some respects the two are quite distinct, but evidence of pressure is clearly to be seen in both. [1]

With this the matter seems to have been dropped except a general unrest among scientists, into whose hands the specimens came, that the results obtained should not be final.

The late Professor W. C. Kerr, State Geologist, made a collection of these specimens with a view to making an examination of them but did not live to do the work. That the subject might be further investigated, Professor J. A. Holmes, State Geologist, visited the region and collected a large number of specimens. For the same reason, Professor Collier Cobb, at whose suggestion and under whose direction I make this examination, obtained an original specimen collected by Professor Emmons, from the Massachusetts Institute of Technology.

With this brief history of the palæotrochis, let us examine its character, mode of occurrence, etc.

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1. American Journal of Science, Vol. XLV. p. 219.

The specimens I have used are those collected by Professor Emmons and Professor Holmes, referred to above.

The rock mass in which the specimens occur is granular quartz of a dark color and splits roughly along apparent planes of bedding. The weathered surface of the rock is very rough, consisting of the protruding fossil-like forms and cavities out of which they have weathered, interspersed with more or less even patches of weathering concretions.

These protruding forms are composed of quartz of a light gray color, sometimes brown from oxides of iron. Around each of these forms there is a ring of softer grayish white material, which in many cases has weathered below the general surface of the rock, leaving the forms standing up apparently in little cups. The concretions in weathering are also grayish white and are circular in section, showing concentric structure about very small neuclei. The fossil-like forms of the palæotrochis are larger than the concretions. I counted in an average specimen, twenty of the palæotrochis exposed in an area of nine square inches, which averaged about three-eights of an inch in diameter, never varying much from that size. The concretions were somewhat more numerous and considerably smaller. The palæotrochis is not distributed evenly through the rock nor does it occur in definite planes of stratification. The individuals are turned in no definite position with respect to each other, but with very rare exceptions one apex or the other of the double cones rests on bedding planes or on planes parallel to these—not with the axis perpendicular to the plane but inclined at a varyingangle depending upon the flatness of the form. Inother words, their general position is

that which they would assume if left under the influence of gravity to collect under water. Figure 10 is a section at right angles to the bedding planes showing their mode of occurrence.

The rock mass clearly shows evidence of considerable pressure at right angles to the bedding planes. This is not only shown by the mass, but often by individual specimens as shown in Fig. 11, which, lying in the position it would naturally assume, received the pressure from above, giving it its present form with the vertical plane of fracture which has since been filled. Owing to the shape and position of the specimen, the pressure would be unequally distributed, fracturing it in the direction shown for the obvious reason that the greatest stress was in that plane. Lines of fracture are also found in microscopic sections of specimens which do not show it externally, Fig. 12.

In form, isolated specimens and those exposed on the weathered surface of the rock, answer to the description of Emmons, given above (pages 50, 51, and 52; also see figures 2–7). But when the weathered surface is broken away and a fresh surface is exposed, these forms are enveloped in a gray, translucent, radial fibrous mineral, which under the microscope proves to be an impure chalcedony. This, as we would expect, weathers gray-white on exposure [1] and is washed away faster than the surrounding rock. The interior, or palæotrochis proper, I find is graunlar quartz as did Marsh. The small concretions prove to be chalcedony throughout. There is a definite line of separation between the chalcedonic formation and both the surrounding rock on the outside and the enclosed palæotrochis.

1. See page 54; also Text Book of Geology. A. Geike—Third Edition. 1893. p. 69.

The concretions are also distinct from the rock mass. Figure 13, taken from a microscopic section, roughly illustrates these points.

Having the general characters of palæotrochis before us, let us now compare it with the forms that it has been thought to resemble and examine more carefully the more minute details.

(1) Is it a concretion? "There is a general tendency in matter, when solidifying to concrete around centres. These centres may be determined (1) by foreign substances which act as nuclei, or (2) by the circumstances of solidification, which according to a general law, favor a commencement of the process at certain points in the mass, assumed at the time. As the solidifying condition is just being reached, instead of the whole simultaneously concreting, the process generally begins at points through the mass; and these points are the centres of concretions into which the mass solidifies.

"The concretions in the same mass are usually nearly of equal size; hence the points at which solidification in any special case begins are usually nearly equidistant.

"In a concretionary mass, the drying of the exterior, by absorption around, may lead to its concreting first. It then forms a shell with a wet unsolidified interior. The interior may then dry, contract, and become cracked; or, it may undergo no solidification, and remain as loose earth; or, it may solidify by the concreting process, forming a ball within a shell, with loose earth between."[1]

If the forms we are considering were even spherical,

1. Dana's Manual of Geology, page 628.

the most general concretionary form, we should still have no difficulty in deciding that they do not belong to the last class, since the interior is a compact mass of semi-crystalline quartz, often showing the layers in which it was laid down by the ordinary process of deposition. (Fig. 14.) From what has been said of their distribution (p. 54), they could not belong to the second class. The chalcedonic envelope is distinctly concretionary, and regarding the palæotrochis as a nucleus, they *can* belong to the first class. But the palæotrochis itself is no ordinary concretion, which Marsh admits and tries to find some analogy between it and cone-in-cone structure.

(2) Is it stylolites or cone-in-cone? "Stylolites are cylindrical or columnar bodies varying in length up to more than four, and in diameter up to two or more inches. The sides are longitudinally striated or grooved. Each column usually with a conical or rounded cap of clay, beneath which a shell or other organism may frequently be detected, is placed at right angles to the bedding of the limestones, or calcareous shales through which it passes, and consists of the same material. This structure has been referred by Professor Marsh to the difference between the resistence offered by the column under the shell, and by the surrounding matrix to superincumbent pressure. The striated surface in this view is a case of 'slickenslides.'"[1]

It is true that the palæotrochis shows signs of pressure, but, as already pointed out (p. 55), the pressure had a tendency to deform the structure and obliterate the grooves or striae instead of forming or constructing them, while the layers of deposit of which the forms

1. Text Book of Geology. A Geikie. 3d. Edition, 1893, p. 316.

are composed, show that there was no pressure when forming. (p. 56, also Fig.14.) Neither are they in the slightest degree similar in form.

"Undoubtedly few of the structures classed under the general head of concretions are more curious than cone-in-cone. The name is descriptive, the structure consisting of corrugated or crenulated conical layers, one within another, and in the more complex specimens it is seen that thin layers of the rock, a calcareous and sideritic clay, is composed of the closely crowded nests of cones, the axes of the cones being transverse to the bedding planes. The height of the cones measures the thickness of the layers, which is commonly one to four inches. It seems necessary to suppose that during the compression of the layers of clay by vertical pressure it is divided by an indefinite series of conical gliding surfaces, which are corrugated by the intermittent character of the movement."[1]

"Clay iron stones sometimes exhibit the regular structure known as cone-in-cone, in which case the seam has a tendency to divide into cones, the bases of which are towards the top and bottom of the bed, while their apices are directed towards the center."[2]

By comparing the character and mode of occurrence of palæotrochis with cone-in-cone, it is seen that there is no similarity between them, but the quotations are given in full to show that no inorganic form has yet been described which explains the origin of palæotrochis.

The palæotrochis is not two cones applied base to base, that simply roughly suggests the general form, and its failure to conform, even approximately, to a

1. Dynamical and Structural Geol., W. O. Crosby, p. 278.
2. Ore deposits, J. A. Philips. p. 165.

geometric figure and its freedom from rigidity in its appearance, either in form or markings, suggests that it is not of inorganic origin. And yet the persistence in conforming to a general outline, in the radial groovings, and the rounded knob at one apex with the smooth cavity at the other makes the conclusion irresistable that it is not "accidental," that they were all formed under like conditions and in accordance with certain laws, and no mode of crystallization or wholly inorganic arrangement can be conceived that will supply the conditions or suggest the laws capable of imitating these forms. The rounded knob and the cavity opposite are very striking. I have examined many isolated specimens and without exception the knob and cavity are present. Those exposed on the surface of the rock, as has been pointed out, generally show one of the apices, and of those I have examined—upwards of 300 in all—not one failed to present either a knob or a cavity,[1] with the exception of not more than four whose apices had been so crushed by pressure that these characters had been destroyed, or the knob simply broken off as in figure 11. I have never yet found one that had a knob at each apex or a cavity at each apex. These two markings seem to be as persistent and as characteristic as the two valves of a brachiopod. The cavity has the exact appearance of the socket of a ball and socket joint. The inner surface appears perfectly smooth under a magnifying glass and vividly suggests that it has been the seat of an organ or of an organism.

(3) It has been shown so far that the palæotrochis

1. This statement applies only to those which present an apex and not to those rare exceptions that do not present either apex.

is not similar to any concretion or class of concretions heretofore described and that it has no anology to cone-in-cone or stylolites. Let us now examine the evidence by which Professor Marsh came to the conclusion that it is not a coral and see if he was justified in that conclusion.

His conclusion, given on page 52 of this paper, is not drawn from the form or external markings, but when he examined the interior with a microscope and found no organic structure he deemed the evidence sufficient and concluded that in the future this name should be dropped from the genera of fossils. If he had found organic structure of course the proof would have been direct and positive, but the absence of organic structure is by no means a proof that it is of inorganic origin, for Nicholson and Lydekker say, in speaking of replacement by silica, the following: "In a large number of cases of silicification, the minute *structure* of the fossil which has been subjected to this change is found to have been more or less injuriously affected, and may be altogether destroyed even though the *form* of the fossil be perfectly preserved. This is the rule where the silicification has been secondary, and has taken place at some period long posterior to the original entombment of the fossil in the enveloping rock."[1]

Therefore it appears that Marsh's determination can not be relied upon.

(4) Was Professor Emmons justified in his statement that it is a coral? It is true the general form, the radiate striae, or grooves, and what he took to be

1. Manual of Palæontology—Nicholson and Lydekker. Vol. 1. p. 7.

the method of reproduction as described above (p. 52) might have seemed to him more suggestive of the coral than of any known form, organic or inorganic, yet the proof is not positive and I can find no just ground for his position.[1]

Admitting the organic origin of palæotrochis, how could it have been preserved? Considering its distribution through the rock mass and the position assumed by the individuals, with the material in which they are imbedded, the explanation is possible by different methods. The first that occurs to me, and which is offered merely as a suggestion is this: The indviduals dropped to the sea floor and were imbedded in ooze. This ooze being largely calcareous but containing a considerable amount of silica on beginning to solidify would have formed in it, silicious concretions, just as they are found in the chalk beds of England. Their origin is explained as follows by LeConte: "Nodular concretions seem to occur whenever any substance is diffused in small quantities through a mass of entirely different material. Flint nodules in chalk. Carbonate of lime modules in sandstone, &c."[2]

As concretions start around nuclei which are generally of a different material from the concreting substance, and are particularly favored by decaying organic matter, it is quite natural to expect these bodies of organic matter to be encased with silica at the same

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1. I have found several specimens that answer to Professor Emmons's description of the process by which they are reproduced, and at apparently different stages of the process. One of the best, which was not in Emmons's collection, I have shown in Fig. 15. This answers better to his description than his own figures, 2 and 4.

2. LeConte's Elements of Geology, p. 188.

time the small spherical concretions are being formed about minute nuclei. Chalcedony often encloses organic forms and so perfectly that the colors of the plants thus encased are preserved.[1] After a great lapse of time let all the calcareous matter of the deposit be replaced by silica, and then follow a period of pressure and uplift and you have the form as it occurs to-day.

That solutions pass through chalcedonic envelopes is shown in water geodes and in geodes containing bitumen.[2] In this view we would expect the cast of silica in the shell of chalcedony to be purer than that which surrounds these forms. This is observed to be the case with palæotrochis. (p. 54). And from the rule of replacement by silica (pp, 60, 61) we would be surprised to find internal organic structure.

It is not the purpose of this paper to assign these forms to any class or order, not even to show whether they are animal or vegetable. But in passing we may note certain classes of organisms, to one of which it may be referred at some future time.

(1) It might have been a calcareous sponge whose spicules were destroyed by replacement. Though as no spicules have yet been found, it can not be put down as a sponge.

Figure 16 represents a sponge similar in form to the palæotrochis.[3]

(2) It may belong to the class hydrozoa.

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1. Transactions of the Geological Society, Vol. II. First Series. p. 510.

2. Dynamical and Structural Geology, W. O. Crosby, p. 275.

3. Ward's Catalogue. p. 205. For description see Transactions of the Geological Society. Vol. 1. p. 337.

(3) It may be an organ of some animal. The form represented in figure 17 is a "cast of what Nathorst considers to be the radial canals of a species of a craspedot Medusa, belonging to the family Æquordae." [1]

(4) They may prove to be of vegetable origin; another variation of the many and striking forms assumed by sea plants.

From the peculiar nature of the knob and cavity, I offer as a bare suggestion that in their original growth they were probably joined togther as in Fig. 18.

These are mere suggestions to show that the palæotrochis is not wholly unlike all organic forms, though it can not yet be assigned a definite place among organisms.

The purpose of this examination has been to call attention to the work done on the palæotrochis, to investigate the methods by which the results were obtained, and to see if the conclusions reached would stand the test of an examination made in the light of more recent discoveries and by more modern methods.

I claim (1) that neither Emmons, Hall, nor Marsh made that careful and scientific investigation of these forms necessary to justify the conclusions reached, and that these conclusions should not be accepted. And I claim (2) that the weight of evidence in the present state of knowledge indicates that the palæotrochis is of organic origin. The reasons briefly summed up are:

(1) Its distribution in the rock. (p. 54)

1. 10th Annual Report, U. S. Geological Survey, 1888-'89. Plate op. p. 676.

(2) Positions assumed by the individuals. (pp. 54, 55).

(3) Their conformity in shape to a general type as persistently as that of any class of organisms. (pp. 51–58).

(4) The failure to conceive of any inorganic process by which such forms could be produced. (p. 59).

(5) That they attract concreting material and are the nuclei of concretions. (pp. 54, 56).

(6) Their general resemblance to determined organic forms. (pp. 62, 63).

(7) An apparent method of reproduction, (pp. 52, 61).

It may take years of patient examination to find direct and positive evidence by which the palæotrochis may be referred to its proper place among organisms, and such evidence may never be found, yet I believe the importance of the subject justifies a much more extended and careful examination than it has yet received.

EXPLANATION OF PLATES.

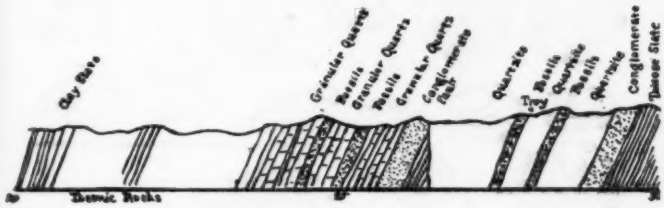


Fig. 1. Section through region of Palæotrochis Beds. After Emmons.

Figs. 2, 3 and 4. *Palæotrochis minor.* After Emmons.

Figs. 5, 6, 7. Specimen found by Emmons, in possession of Prof. Collier Cobb.

Figs. 8 and 9. *Palæotrochis major.* After Emmons.

Fig. 10. Showing position of fossils in bed.

Fig. 11. *Palæotrochis* deformed and fractured by pressure.

Fig. 12. Microscopic section showing fracture.

Fig. 13. Microscopic section showing chalcedonic envelope.

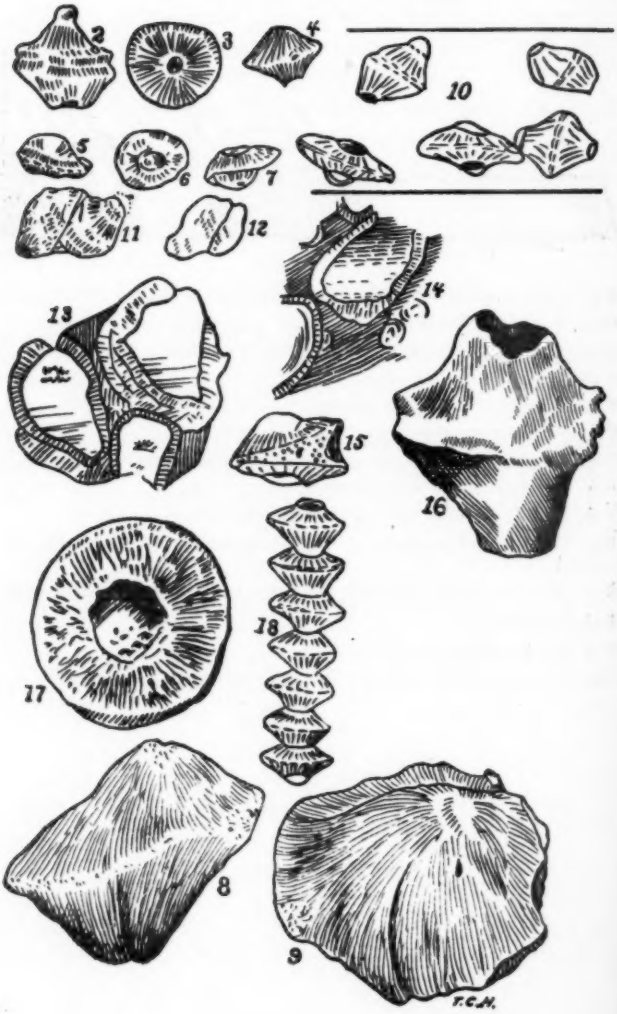
Fig. 14. Polished surface of rock showing layers of deposition in specimen.

Fig. 15. *Palæotrochis* showing what Emmons described as budding.

Fig. 16. Sponge.

Fig. 17. An organ of medusa.

Fig. 18. Suggestion as to possible arrangement of *palæotrochis.*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THE ATOMIC WEIGHTS AND THEIR NATURAL ARRANGEMENT.

F. P. VENABLE.

It is proposed in the following paper to emphasize the necessity for the acceptance of oxygen as the standard for the atomic weights, to point out the fact that their absolute determination is not within the bounds of reasonable hope; to show the folly of speculations as to the primal elements in the present state of our knowledge; and to suggest certain changes in the Periodic Arrangement of the elements as given by Mendelejeff.

THE OXYGEN STANDARD.

In 1888 the writer of this paper published in the JOURNAL OF THE ELISHA MITCHELL SCIENTIFIC SOCIETY, (Vol. V. 98.) a plea for the adoption of Oxygen as the standard for atomic weights—a return to the very wise and scientific usage of Berzelius. A reprint of this paper was sent to the *London Chemical News* and must have been in the hands of the editor of that journal when there appeared in its pages a paper by Dr. Bohuslav Brauner, of Prag, upon the same subject. The reprint mentioned was published in the *Chemical News* a week or so later. The same standpoint was taken in the two papers and largely the same arguments used. Indeed it seemed almost incredible that the two could have been written entirely independently of one another. These facts were adverted to by Dr. Brauner in a subsequent paper in the Berliner Beri-

chte (Ber. deutsch. chem. Ges. XXII. 1175.) when the question was being discussed between Meyer and Seubert, Ostwald and himself.

This was six years ago. The matter has not been much discussed in the mean time. Still the desired result has been partially attained. Many chemists seem to have adopted the oxygen standard and it is made use of in most recent work in this line. Some have spoken of this as only a temporary abandonment of the hydrogen standard. This can be true only in case the chief argument for the oxygen standard ceases to be valid. This argument is, that, in the majority of cases the atomic weight determinations involve combinations with oxygen and hence the use of its atomic weight in calculations. This weight should by all means be fixed and not dependent upon determinations of the ratio to hydrogen or any thing else, to be upset every few years by new and "more accurate" determinations. Only in two cases can hydrogen replace oxygen as the standard. First, in case suitable compounds of the various elements and hydrogen can be obtained. This does not seem very probable. The second case is where absolute accuracy of determination is conceded as impossible and the final atomic weights can be settled upon by some methods of mathematical calculation. Such methods have been suggested but their adoption does not seem probable. It is scarcely necessary to point out that the use of O as 16 or O as 15.96 would make a very marked difference in the cases of elements of high atomic weights—several integers for uranium for instance. Oxygen as 16 must remain the standard for the present and it will be so considered in the remaining portion of this paper and uniformity in this regard is very earnestly to be

pressed upon all who desire the advancement of the science.

HOW FAR IS ACCURATE KNOWLEDGE OF THE ATOMIC WEIGHTS ATTAINABLE?

The atomic weights are generally considered the most important constants in chemistry and yet so imperfectly are they known and so varying the numbers assigned them that it has not been possible so far to settle finally whether they really are constants or variables within narrow limits. The probability, however, is so greatly against this latter view that there are few who are inclined to accept it. As more than three quarters of a century of work has been expended upon them, work engaging the utmost efforts of the masters of the science, as Berzelius, Dumas, Marignac, Stas and many others, it may with perfect justice be asked whether absolute accuracy is attainable.

Some have hoped that the more perfect knowledge of the chemists of the present, the better methods of separation, purification and general manipulation, and the fine balances, would enable them to attain to the desired accuracy. There have been, of course, many improvements but any one who will carefully examine the determinations of Berzelius will find many of them in marvellous agreement with the finest work of late times and when he goes over the list and sees for how many of these atomic weights the work of Berzelius is still relied upon as the best, he will be less boastful of the progress and less hopeful of results from it.

Certainly, if accuracy is to be attained, then the usual method of those who re-calculate these weights, and in fact the only allowable method at present,

namely, that of taking the work of different authors, and critically averaging it, must be rejected, for this would never secure concordant results as they would have to depend upon the judgment of the critic and calculator. All of this back work must be wiped out of existence, except for historical use, and we must begin anew with every conceivable refinement of method and apparatus, perhaps devoting, as has been suggested, some central endowed laboratory to that work and that alone.

Calculations of "probable errors" have given a seeming accuracy to many atomic weight determinations. In this chemists have followed the lead of Stas. It seems to me that this is very misleading, these calculations often being made upon small series in which the possible error, as shown by the variation in individual experiments, is ten times that shown by the calculation. In the proposed new determinations this method of calculation should only be allowed in the case of several hundred closely agreeing determinations.

Of course, it might as well be confessed that absolute accuracy is not to be even hoped for. The best methods and appliances which can be devised or manufactured will always be imperfect and there is besides the personal error of the observer to be allowed for. To what extent shall we demand accuracy, then? Where shall the line be drawn? Is it to be at the first decimal place or the second? It seems useless or hopeless to speak of the third. There can scarcely be said to be an atomic weight at present known correctly to the first decimal place. Take the numerous determinations for oxygen, exceedingly modern and excellently well carried out, and see how they vary between 16.0, 15.9 and 15.8 and look at the original series from

which these results were calculated and see how they are but the means of series with decidingly varying figures—a balancing of errors perhaps. One is induced to think that for many years, at any rate, the highest attainment to be hoped for will be a correct first decimal.

These being the facts, it is but false pedantry in the present state of our knowledge to write these figures beyond the integer in most cases unless the decimal is a large one. In a few cases the first decimal might be used. In no case is the second justifiable. What would be gained by a knowledge of the absolute atomic weight beyond the satisfaction of having secured that much knowledge of the truth? Do these costly labors promise results sufficiently valuable to justify them or are the energies of many of the most skilled chemists misdirected and wasted? Of course no true labor is wasted, but an energy which would accomplish grander results in some other direction is wasted if turned into trivial channels. All will acknowledge that no good to practical chemistry would result from weights known to the second decimal place. If known approximately to the first every requirement of the analytical chemist would be satisfied.

Two things may be gained for science however by such knowledge. First, the question of constancy of weight would be settled, beyond all reasonable doubt, and secondly, data would be obtained for the discovery or confirmation of underlying laws.

Some dim presentiments of such laws were seen when these weights were in their most chaotic state. The vision of them was obscured by the confusion of standards and the disagreement in determinations. As these difficulties were partially removed the way

became clearer for the discovery of the inter-relation of the elements and the dependence of their properties upon their atomic weights. It must be remembered that for several of the elements the atomic weights are unknown and for others are very poorly determined. It is extremely important that these be correctly determined, and work spent upon them is far from wasted, but I must confess that I can not but feel that further efforts to discover the ratio between hydrogen and oxygen are of little value to science and that chemists generally would be more grateful were the same labor devoted to such elements as thorium, cerium, or nickel, and many others.

The question of the variation of the elements in their atomic weights is a very elusive one and scarcely capable of being finally settled by even the most accurate work of the chemist. The supporters of the hypothesis have always a loop-hole of escape in the limits within which these numbers may be supposed to vary. This variation is now narrowed down to the decimal places. As the determinations become more accurate, it is easy for the limit to be moved from one decimal to another and so defy pursuit. Nothing but absolute accuracy, an accuracy shown in every experiment, with all sorts of varied proportions, and not an averaged result, could finally end the discussion. But with a settled standard and the atomic weights known to the first decimal place, the way would be clear for laws dependent upon their inter-relation.

APPROXIMATION TO WHOLE NUMBERS.

Speculations upon the numerical relations existing between the atomic weights began almost with the

first imperfect list of these weights. These took two directions. First the ratios to some common standard or unit as hydrogen, and secondly the relations between the weights of elements of the same group or family. The first subject was looked into simultaneously in the year 1815 by Prout in England and Meinecke in Germany. The second was naturally taken up some years later, the first to suggest numerical regularities being Döbereiner in 1817 and he has been followed by a number of others.

Prout's hypothesis has always attracted the most attention. It may well be divided into two parts: first, an assumption that the atomic weights are all whole multiples of hydrogen. This was afterwards modified so as to read that they were all integral multiples of the half atom of hydrogen. This half atom, or rather body having half the atomic weight, was called pantogen. The second was a deduction from the first assumption. If they were multiples of hydrogen, then they must be composed of hydrogen or of pantogen. Why this should be the case or was at all a necessary deduction no one seems to have attempted to show.

The first assumption has been examined and worked over by many investigators with a view to proving its truth or falsity. If proved true, it would be interesting and useful, but it could never justly be claimed as showing that the elements were formed of hydrogen or the hypothetical pantogen. If we take the list of atomic weights as calculated by Ostwald and select those in regard to which we can feel sure that the weight is approximately correct and if we disregard variations of less than one tenth from the unit, then we find that twenty-three out of thirty-five are integral

multiples of hydrogen. Clarke claims forty-one out of sixty-six but includes such as niobium, didymium, gallium, tungsten, thorium, &c. A little critical examination of his list will easily cut the whole number down to little more than half of the forty claimed. The best that can be said is that about two in three are whole numbers and the remainder run the full range of decimals from .1 to .9 and no halving of the atomic weight can possibly hit upon them. When it is considered that about as many of the elements are whole numbers when hydrogen is taken as 1.0025 as when it is equal to unity, it will be seen how little bearing upon hydrogen as the primal element the facts of integral atomic weights would have.

THE IMPROBABILITY OF HYDROGEN BEING THE PRIMAL ELEMENT.

I hesitate to discuss this question because I scarcely think it is seriously urged but a few thoughts may not be amiss. The supposition of a primal element having as its atomic weight half or any other fraction of the weight of hydrogen is based simply upon the increased number of coincidences of the atomic weights with whole multiples and can have little weight. Such an hypothetical pantogen escapes all serious argument. But against the supposition that hydrogen is the primal element many things may be urged.

In its favor there is little beyond the fact that some two-thirds of the different atoms, so far accurately determined, have approximately integral weights and that, in the table of elements, hydrogen occupies a most anomalous position and refuses to be satisfactorily arranged in any of the groups or periods. If,

however, we are to judge of this matter by ordinary rules, it seems highly improbable that an element of such definite, postively marked characteristics can by any kind of condensation or combination be changed into such markedly opposite bodies as as fluorine and sodium or chlorine and potassium. We are coming more and more to regard an element as representing an assemblage of properties. Thus chlorine stands for a form of matter, gaseous and most energetically negative whilst a slight increase of weight brings us to potassium a solid metal and most energetically positive and it is quite unlikely that this should be due merely to a small additional condensation of such a body as hydrogen. It is contrary to the gradual change of properties observed in cases of polymerism or even homology in organic chemistry.

The supposition of two or more primal elements, condensed in varying proportions, is in accord with phenomena known to us but of course is so far without experimental or other basis if we exclude the mathematico-spectroscopic work of Grünwald. Take for instance, the widely different results obtained by varying the ratio between nitrogen and hydrogen in their compounds. Thus 3N and H give a well characterized acid and N and 3H give an equally definite base. This complete reversal of properties can no more be attributed to the hydrogen alone than to the nitrogen. The primal elements might act in this way in their condensation into the common elements.

There is no basis for the formation of any hypothesis as to the primal elements and speculations on this score are as idle as the dreams of the early Greek philosphers. The future may bring such knowledge as will afford the needed data. Certainly we are a long

step nearer to it in the recognition of the fact that the properties of the elements are dependent upon and determined by the atomic weights.

THE PERIODIC LAW.

A quarter of a century has passed since the first announcement of the Natural Law and the publication of Mendelejeff's table. The truth of the law in a general way seemed to be accepted very readily by chemists. It was incorporated in text-books and there explained, but comparatively little use has been made of it in teaching the science. Even Mendelejeff himself, in his *Principles of Chemistry*, has not made the fullest use of it. Victor Meyer, in his lecture before the German Chemical Society more than a year ago, showed how it might be used and how he used it himself, and probably, this will do much toward popularizing its use.

There must be some reason why so great a help to scientific study is not made more use of. Does it lie in a lingering distrust of the law itself or failure to accept it or is it because of the imperfections in the arrangements of the elements offered by Mendelejeff and others? It is most probably due to the latter and this paper is presented with the hope of clearing up some of these difficulties.

The modern chemical world has recognized in the discovery of Mendelejeff the greatest step forward since the announcement of the atomic theory. It is too much to expect that so great a discovery should spring full-panoplied from the head of its author. It has been accepted by chemists in all lands and is the basis of present chemical thought. Doubtless many have observed the im-

perfections of the law's original form or rather the table as first brought out. Probably some have ventured to comment upon it. Such criticisms have escaped me with one or two exceptions.

It is with much hesitation that I venture to point out what seem to me to be imperfections and blemishes in so great a work. Few may agree with me in calling them imperfections. I do not purpose to detract one particle from the greatness and importance of the essential truths contained in this discovery. Mendelejeff's table, as we have it at present, is a great advance upon the first one published by him in 1869 which must be pronounced tentative only and decidedly unsatisfactory. The table of Victor Meyer is far behind it in presenting the facts of the periodic law. There have been many attemps at devising a graphic representation of this law. I know of none which can be called real aids to the student or which do not introduce new ideas which, to say the least, have no basis in the facts as known to us at present. None of them can be regarded as a safe substitute for the simple table of Mendelejeff.

Taking that table I would venture to point out some obstacles to its present full acceptance. These have been in part revealed to me by the effort at an honest presentation of this great truth of nature to honest-minded, clear-sighted young men. Before mentioning these difficulties which lie here in the path of a teacher, I must preface that my criticisms are aimed at what I may be allowed to call the unessentials of the law. Mendelejeff's great feat was in seeing clearly and announcing intelligently that the properties of the elements were dependent upon and determined by the atomic weights. This is the essential of the periodic

law and is in accord with our fullest knowledge. The second part of the law as usually stated, that these properties are periodic functions, attempts in a measure to define the dependence, It may also be true but it is not fully proved and is open to objections. It seems to me that this hypothetical portion could well be left in abeyance until fuller knowledge gave it a stronger footing, meanwhile substituting something less open to criticism and which cannot weaken the central truth.

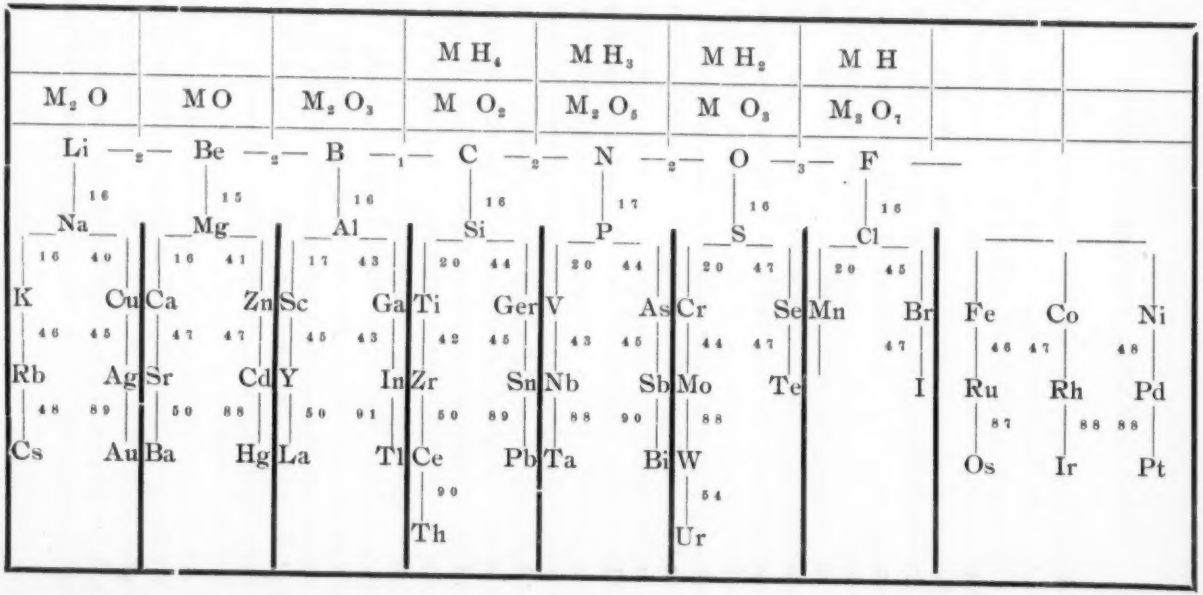
Take this table and examine it. First we find two kinds of periods made use of—periods containing seven elements and those containing seventeen. If it had only been possible to arrange all of the elements in sevens as Newlands attempted to do, the periodic idea would have been most convincing and the law of octaves running through nature would have seemed most wonderful. But these elements do not admit of being arranged in this way and the use of periods of different lengths is to fresh young minds, unacquainted with mathematical expedients, somewhat forced.

Secondly, there is a very anomalous position assigned to the triads or, as sometimes written, the tetrads, Fe, Co, Ni, (Cu,) etc. They have been set off to themselves, clearly so as to make the other elements fall even approximately into their places and into the proper sevens. I say approximately, for the student soon sees that although there is a similarity there is also a wide difference between the elements of the first and of the last seven in any period of seventeen.

Thirdly, in the lower periods, in order to get elements to fall into their places a great many unknown elements have to be interposed. Thus between cerium and ytterbium, the next element in the list, there are blank places for sixteen elements. The third large

period of seventeen has only four known elements in it and the fifth has only two. Of the five periods only one is completely filled out. To say the least, this shows a very imperfect knowledge of the elements, or a great deal of guess work. In the table there are sixty-four known elements and thirty-five blanks for elements yet to be discovered. I hardly think it possible that the majority of chemists believe that after all of our diligent search for the past century less than two-thirds of the elements have been discovered. Where are the others in hiding? Will they be discovered by the spectroscope in the rare earths? There is certainly hope of finding some but the number to be found is appalling. The average student thinks, in all honesty, that the coincidences of the first part of the table will scarcely justify such forcing and wholesale interpolation. If our knowledge of the elements is as imperfect as that, we have no right to force them into periods. Some of them seem little inclined to fall into these periods of their own accord. How do we know that the remaining two-thirds may not upset the entire calculation? Certainly we are venturing a good deal upon a very imperfect knowledge of the remainder. Let us see how the matter stands. The periodic idea may be true but we do not know enough about these elements yet to be able to give this idea a very prominent place in the natural law, and we ought to avoid the assumption of so many unknown elements unless absolutely necessary.

As I do not intend to tear down without some effort at re-building, I would, with much real diffidence, for I realize that I may be looked upon as one who would rush in where only the great masters of the science can safely tread, offer the following table as a substitute:


M H4
M H3
M H2
M H
M2 O
MO
M2 O3
M O2
M2 O5
M O3
M2 O7
Li —2— Be —2— B —1— C —2— N —2— O —3— F —
16
Na
15
Mg
16
Al
16
Si
17
P
16
S
16
Cl
16 40
17 43
20 44
20 44
20 47
20 45
K
Cu
Ca
Zn
Sc
Ga
Ti
Ger
V
As
Cr
Se
Mn
Br
Fe
Co
Ni
46 45
47 47
45 43
42 45
43 45
44 47
47
46 47 48
Rb
Ag
Sr
Cd
Y
In
Zr
Sn
Nb
Sb
Mo
Te
I
Ru
Rh
Pd
48 89
50 88
50 91
50 89
88 90
88
87 88 88
Cs
Au
Ba
Hg
La
Tl
Ce
Pb
Ta
Bi
W
Os
Ir
Pt
90
Th
54
Ur

My first suggestion is that the wording of the Natural Law be so changed as to read: "The properties of the elements are dependent upon and determined by the atomic weights." The somewhat difficult idea of function is simplified and periodicity is subordinated. Then I would substitute the following table for that ordinarily given. It is not greatly changed, and not much originality is claimed for it, but however slight the changes I would insist upon their value, because they do away with the dependence upon periods, and they certainly make the table an easier, more intelligible, and more useful one to the student. There is no forced effort at rounding off any period or group. There is room for additional elements when discovered, but the table is not dependent upon them.

Lastly, the inter-relation is more clearly brought out. I do not maintain that this table could ever have been discovered without the idea of periods, though I think it might. The periods still underlie it, but they are out of sight for the present and are not necessary. The table is not dependent upon them.

There are seven group elements having a mean increment of two in their atomic weights. It is by no means essential that there should be just seven of these. At present we do not know more, but I think there is possibly a place for one more having the atomic weight 21, differing widely from the others as it occupies a singular position.

These group elements are also to be called bridge elements as they show marked gradation of properties from one to the other and so serve to bridge over the groups and connect one with the other. Linked to them by an increment of sixteen are seven typical elements. These show the distinctive properties of the

groups to which they belong and a wider divergence from the next group to them. From them can be deduced the properties for the remaining elements of the group. Thus in group I Li is the bridge or group element and Na the type. From this type two lines of elements diverge, averaging three to the line. These triads would of course be changed into tetrads or pentads by the discovery of more elements. No importance is attached to the fact that at present they are in threes. There is a distinct increment for each line of elements. These can be averaged thus; Fig. 1 representing the arrangement and increments for the first three groups, and fig. 2 the arrangement in the last four groups, the increments varying slightly. These increments could be averaged in all except one case, and the agreements with known atomic weights would be close enough to admit of the easy arrangement of the elements in the prescribed order. Naming the triads Right Triad and Left Triad respectively we find that these averaged increments would be as follows: the increment from group to type element is sixteen; from the type to the first element in the Left Triad (L. T.) is 18; to second element L. T. is 63; to the third is 112;—to the first element to the Right Triad (R. T.) is 41; to the second R. T. is 88; to the third is 177.

Fig. 1.

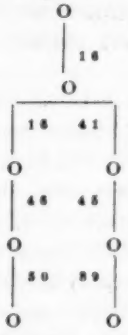


Fig. 2.

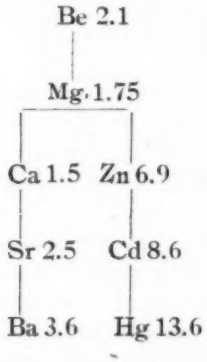
The one exception is in the increment from Type to III L. T. from group IV to VII. Instead of being 112 this is 141.

To the right of Group VII we have three triads which have nearly the regular increments belonging to the Right Triads, namely, 47 and 88. They are without any type element, it seems most likely that they belong to one group. The Group element would have an atomic weight of 21 and the Type one of 37.

The arrangement in the table then is partly one based upon regular increments in the atomic weights, and since these are so poorly known, partly upon our knowledge of the chemical properties of the elements. When it is recalled that about one half of the atomic weights are imperfectly known it will be evident that these averaged increments are approximations only. It is impossible to bring out such perfect symmetry as obtains in the homologous series in organic chemistry. And yet these groups should be something of the same kind. Following the analogy to the organic hydrocarbons a little further, may not the existence of an element in two different conditions as to valence, &c., as,

for instance, copper or mercury or iron be looked upon as a species of isomerism. Such speculations are of little use, however, and quite apart from our present purpose.

I have found this table very useful in teaching elementary chemistry and it can most profitably be made the basis of the entire course. Thus in the first four groups the left triad contains the elements most closely resembling the Types. In the last three they are to be found in the right triads. As to natural occurrence of the elements, in the first four groups those in the left triads occur in the same compounds and generally in connection with the type; those in the right triads occur as the type or as sulphides or are free. In the last three groups this is reversed. The right triad elements occur as the types and the left triad as the type or as oxides. So too the properties of the elements show this relation to the types. Take as an example the specific gravities in Group II.



It is not necessary to pursue this part of it at greater length. The careful teacher will easily work out

all these comparisons for himself and will find that chemistry taught by the table is shorter (so much repetition being saved) and is easier for the pupil and its symmetry and beauty is much more easily brought out. There is no special claim for originality made here. The germs of such a table or arrangement can be found in several text-books but I do not know of any in which the idea is fully developed or such a table as this is given.* I offer the whole as a suggestion. Perhaps some may find it useful who have met the same difficulties which I have encountered. Others may have overcome these difficulties in a still better way than this, and yet others may see no difficulty at all in the present table. I think, at least, all will agree with me that there are difficulties and very genuine ones also in the use of Mendelejeff's or Meyer's tables as given by the respective authors.

IMPROVEMENT IN THE METHOD OF PREPARING PURE ZIRCONIUM CHLORIDES.

CHAS. BASKERVILLE.

The preparation of pure zirconium chlorides from zircon is a rather long and tedious process. Linneman's method, [*Sitz. Ber. Kais. Akad. d. Wissen-Schaft.* Vol. II, 1885, translated in Chemical News LII, 233 and 240. on "Treatment and Qualitative Composition of Zircon."] which is very long, was much shortened and simplified by Venable [*Journ. Anal. and*

* The arrangements of Bayley, Hinrichs, and Wendt are some what similar but the ideas which I would make prominent, are obscured by other considerations and speculations.

Applied Chem. Vol. V p. 551]. Bailey's method [*Journ. London Chem. Trans. 1886, p. 149*] of precipitation by means of hydrogen peroxide is very expensive, aside from the difficulties one encounters in preparing the reagent pure.

Having learned that zirconium could be precipitated completely, freed from iron and aluminium, [Baskerville *J. Am. Chem. Soc. XVI, p.* 475.] by means of sulphurous acid, when working with the small amounts usually employed in analysis, I proposed to apply the same treatment to quantities in bulk for the purpose of obtaining a pure salt of zirconium.

The powdered zircon, washed with hydrochloric acid, (100 grams) was fused and treated according to the directions given by Venable [*loc. cit.*] up to the point where the impure zirconium chloride had been freed from silicic acid and was in a dilute hydrochloric acid solution.

This solution was nearly neutralized with ammonium hydroxide. A strong stream of washed sulphur dioxide gas was then led into the cold solution to thorough saturation. This required about fifteen minutes. Partial precipitation occurred in the cold, but other experiments had shown that the precipitation would be more complete if this solution saturated with sulphur dioxide was diluted largely and boiled. Five to ten times as much distilled water was accordingly added and the whole boiled half an hour in large evaporating dishes. The zirconium precipitated out and settled nicely. No bumping occurred during the cooking—while hot, the liquid was rapidly gotten away by means of an unglazed porcelain suction filter. The precipitate was washed two or three times with hot water, then boiled in water and again washed after

filtering. This precipitate was then dissolved in dilute hydrochloric acid and boiled to expel most of the sulphur dioxide. The solution was re-precipitated with ammonium hydroxide. The precipitated hydrates were washed free from ammonium salts and then dissolved in concentrated hot hydrochloric acid. Five crystallizations from the strong acid were found to be sufficient to remove the small amount of iron remaining.

Investigations are now in progress concerning the composition and nature of the precipitate produced by the sulphur dioxide.

A NEW POST OAK AND HYBRID OAKS.

BY W. W. ASHE.

During the past two years, 1893 and 1894, I have observed during several trips through the eastern sections of Virginia, North and South Carolina, a large number of oaks morphologically different from any described species, and in most of the cases, where mature fruiting specimens have been secured, the characters have required that they be referred to the already large list of oak hybrids. One of them, however, is a new form of the post oak, distinct enough to merit varietal place, and so described. The other forms, which I have examined, from which late fall leaves, winter buds and fruit were secured, have proved to be hitherto undescribed hybrids; while a large number are hybrids previously observed by others in different parts of the United States and to the elucidation of which my mea-

gre notes can add but little. I do not think the following hybrids have been previously reported from North Carolina: *Quercus Rudkinii*, Q. *sinuata*, Q. *aquatica* x Q. *Catesbæi*, Q. *minor* x Q. *alba*. The last one I find in the middle section of both North and South Carolina. In Mecklenburg county I found a fine specimen of Q. *coccinea* x Q. *phellos*, but its characters do not agree at all with the Q. *heterophylla* as described by Michaux in the Sylva or by Mr. Martindale. I have a large number of other forms from these and other states which I have not yet looked over or from which I have not yet succeeded in obtaining fruit or flowers.

To several of these hybrids, which are most constant in form, distinct in character and which are not intermediate in form between the parents, I have ventured to apply specific designation.

The following are the forms in my herbarium which I have looked over, with a few brief notes describing them:

Q. *cinerea* x Q. *Catesbæi*.—*Form 1*.—The sessile leaves, 3 to 5 inches in length, are narrowly oval to oblong, rarely oval in outline, with three short bristle-tipped lobes at the summit; or oval and entire. They are either rounded or acute at base. Above they are smooth and shining; below, whitened with the close white stellate pubescence of cinerea. The veins are straight and prominent while those of cinerea are obscure. The twigs and buds are coarse and large like those of Catesbæi; but the bark and general appearance of the trees is that of cinerea. The persistent leaves turn, in the fall, first a yellow and then by December a dull brown, at which date the foliage of Catesbæi is scarlet or partly green. The fruit, however, is that of Catesbæi, frequently, with a tumid base to

the cup, or the wall of the cup rolled inward around the margin. These trees are frequent. Ten or twelve were seen, which were essentially alike in foilage; and those which had fruit agreed in that.

Form 2.—The leaves are longer than in the above, 4 to 6 inches long; mostly oblong in outline; always three lobes at the summit, usually with long falcate lobes; either dentate or lobed on the margins. There are some scattering white pubescence of cinerea over the lower surface and tufts of coarse hairs in the axils of the veins. The bark is rough and black. The trees usually fork and have the general appearance of Catesbæi. The leaves turn scarlet in the fall like those of Catesbæi. The acorns vary a good deal but are more generally like those of cinerea. Several of these trees were seen.

Q. *cinerea* x Q. *laurifolia.*—Leaves oval or oblong, 1.5 to 2 inches long, acute at each end; deep green and shining above, below lighter and covered, especially on young shoots, with a scattering appressed tomentum. The leaves of young shoots are variously lobed, especially towards the summit. The midrib is very prominent, and also one or two pairs of lateral veins. No tufts of hairs are in the axils of the leaves. Twigs are covered when young with a thick white pubescence. The trees are small, 15 to 25 feet high, with broad, spreading, globose crowns, rather resembling laurifolia. Leaves remain bright green, or partly turn yellowish by December 6. Nut is brown, smooth, obscurely ribbed, subglobose; cup deep, covering from one-third to one-half of the nut; scales smooth, firmly appressed. Acorns are almost intermediate in character between those of the parents, except in pubescence. The fertile embryos are largely atrophied. Several

trees were found in different places, but always near the coast.

Q. *cinerea* x Q. *aquatica*.—The leaves 2.5 to 3 inches long, 1.5 to 2 inches broad, are broadly spatulate; rounded or three lobed at the summit; rarely rounded at base. Above they are smooth; below varying from the thick, white pubescence of cinerea to nearly smooth, with small tufts of coarse pubescence in the axils of the veins. The venation is mostly obscure. They are small trees, 20 to 25 feet in height, with drooping branches and rather smooth gray bark. The twigs are pubescent save where the pubescence has worn off. On December 2 the trees are nearly naked of leaves, the leaves still persistent being yellow, while type trees of aquatica are green. The nut is globose, brown, not ribbed, tomentose, as are the scales of the shallow or deep cup; cups persistent on the twigs as those of aquatica frequently are. Several specimens of this hybrid were seen, all near the coast.

Q. *petiolaris*.—Q. *cinerea* x Q. *tinctoria*?—The leaves are oblong or elliptical, 4 to 6 inches long and 1.5 to 2 inches broad; sinuate or crenate on the margins; mostly three lobed and dilated at the summit; truncate or subcordate at base. The lobes at the summit are usually bristle-tipped. Leaves are smooth above or with glandular, septate hairs along the midrib; the under surface covered with a close, brownish tomentum, wearing away with age; tufts of coarse pubescence in the axils of the primary veins. The venation is that of the black oaks (tinctoria and coccinea) with 4 to 6 pairs of prominent, impressed primary veins. The petiole is one-half inch long. The long buds, .3 inch, are lanceolate. The slender twigs are covered with the brownish tomentum of cinerea. Five or six

such forms were seen; all small trees with the bark and general aspect of cinerea. By December 1 the leaves have turned a light brown and are mostly persistent. The deep, nearly sessile cup, enclosing nearly one half of the nut, is top shaped and acute at base. The large, obtuse, pubescent scales are appressed or usually so. The cup is strongly spreading just below the margin. The nut is about one third larger than that of cinerea, oval, pubescent; the persistent base of the style is prominent. Only one tree was found in fruit and its nuts were mostly imperfect.

The character of the pubescence on leaves, twigs and fruit, the oval and entire form of a few of the leaves and the general appearance of the tree indicate cinerea as one parent. The other parent is one of the long-petioled leaved black oaks. Rubra, which probably does not occur, or rarely, where this was found, would be excluded by the deep cup. The shape of the leaf points to cuneata, but neither nut, cup or bud agrees in any particular. The forms of nut and cup are clearly toward those of tinctoria or coccinea, and especially does the thick wall of the cup and the angle made by the outer surface just below the margin indicate one of these species. I am inclined to say tinctoria.

Q. *cinerea* x Q. *nigra*.—The leaves are 3 to 4 inches long, 2 to 3 inches broad, usually broadest at the upper end. In shape they vary from elliptical to ovate, rarely slightly three lobed at the summit. The oval leaves are acute at base and usually acute at the summit. The others vary from rounded to cordate at base. Old leaves are smooth above except on the midrib which is covered with the close septate, stellate, brownish-gray pubescence of cinerea; below they are covered with a down of the same color. There is a dis-

tinct petiole one-eighth of an inch long. The venation shows the dichotomous forking so characteristic of nigra. The upper bud scales are pubescent. The leaves are thick and stiff. In the fall they first turn yellow and then dull brown and persist. They are small trees, 6 to 10 feet in height, with drooping branches and rough bark. Only a few nuts could be found, only a few trees fruiting. These were oval and black ribbed, somewhat larger than the nuts of cinerea, and had disproportionately enlarged hemispherical cups, covered with coarse pubescent scales. The trees in November have the general appearance of nigra after the leaves have turned. Perhaps ten such trees were seen, all in the neighborhood of the coast.

Q. *aquatica* x Q. *nigra*.—The leaves are 2 to 4 inches long, 1.5 to 3 inches broad; broadly ovate or deltoid in outline, broadest above the middle. They are scolloped or slightly 3 (rarely 5) lobed and short bristle tipped; sessile and usually acute at the base. Above they are glabrous and also below, except in the axils of the principle veins where there are tufts of coarse hair. The leaves are thick and firm, and on November 24, were mostly green or the lemon yellow which withering leaves of aquatica turn. The venation is that of nigra. The buds are small but hairy at the summit as are those of nigra. The twigs are slender and warty. The pubescent—scaled cup is hemispherical, and encloses one-half of the ovate, obscurely ribbed nut. The nuts are imperfect, often with the fertile embryos but slightly more developed than the abortive ones. The branches are drooping; the bark is slightly gray. Only a single tree was seen, about 20 feet in height and with a broad spreading crown. Imperfect nuts and cups were abundant, the latter usually remaining on the tree.

Q. *dubia.*—Q. *phellos?* x Q.———. Leaves are entire, 3 to 7 inches long, and 1.5 to 3 inches broad. They vary in shape from lanceolate and linear lanceolate to broadly ovate or elliptical. The lanceolate leaves are shaped like those of phellos, broadest at the lower third; the larger ones are obtuse at each end and symmetrical. All are tipped with a single bristle. Above they are smooth; below there is some scattered pubescence over the entire surface and a line of coarse hairs along both sides of the midrib, as is the case usually in phellos. There are many pairs of prominent straight lateral veins. The venation is something like that of tinctoria. The stout petiole is nearly one-fourth of an inch long. The slender buds, .15 inch long, are pubescent, but the twigs are smooth. The distinctly peduncled cup is top-shaped or hemispherical, with involute margin; scales small, bright brown, closely appressed, almost smooth. The cup is .6 to .9 inch broad, .4 to .5 inch deep, and encloses one-half of the nearly globose, black and brown striped, hoary nut. It fruited abundantly.

Only a single tree of this remarkable form was seen. This was in the open, and was about 25 feet tall, with a spherical crown and spreading branches. The trunk, 10 feet long, had a rough, dark bark. The leaves were partially green and yellowish on November 20. They all turn a dark brown and drop. When green it resembles the evergreen magnolia. Although the shape of the leaves does not bear me out, I think this tree will prove to be a hybrid between phellos and tinctoria or coccinea. The texture of the leaves, however, is firmer than in any of those trees. There is no physiological debarment, that I know, which might prevent a third species from entering this combination.

That is, a hybrid phellos x tinctoria might in turn be fertilized by cinerea or nigra, and this tree be the result.

Q. *falcata.*—Q. *phellos* x Q. *cuneata.*—The leaves are 3 to 5 inches long, and 1 to 2 inches broad; oval or oblong to lanceolate in outline. The smaller leaves are entire, ovalin outline, and acute at each end and have a very short petiole; while the largest are lanceolate with several shallow lobes towards the base and a long terminal, frequently falcate lobe, tipped with a single bristle. The petioles are from one-fourth to one-half an inch long. The nearly sessile cup is saucer-shaped, .5 to .6 inch in diameter and .2 inch deep with the closely appressed scales tubercled at base. The cup encloses only the base of the globose or sub globose nut which has the light brown color, in dead specimens, and wartiness peculiar to cuneata. Nuts are mostly imperfect and many, only half-formed. A large tree 60 to 65 feet in height with a large and spreading crown; bole 18 inches in diameter, with a rough dark bark resembling that of phellos. The foliage was a light green on November 20; dead leaves turn at once a dull brown.

Quercus minor var. Margaretta.—The leaves are 2.5 to 3 inches long, and 1.5 to 2 inches broad. They are oval in outline; entire, wavy-margined or with three spreading lobes at the summit. They are mostly acute at base, rarely obtuse or truncate. Above they are smooth, below they are soft downy. The slender petiole is from one-fourth to one-half inch long. The twigs are slender and smooth. The buds are acute and bright red, sharply 5 angled and large, .15 to .2 inch long. The cup is top shaped, rarely rounded at base, sessile or nearly so. The cup is .4 to .55 inch deep,

.45 to .5 inch broad and covers one-half or more of the slender brown nut which has a length of from .7 to .8 inch and a breadth of form .4 to .5 inch. The nut is silky canescent at the summit and is persistently beaked with the long and slender base of the style. This tree fruited abundantly in 1994, and was the only high ground white oak in eastern North Carolina that did do so. They are small trees 20 to 30 feet in height, growing in the high pine barrens of eastern North Carolina. When trees are killed by fire the roots usually sucker freely. The bark is similar to that of the type.